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immigrants. July 12, provisional flag steamship *Tomas Brooks*, from Kingston, Jamaica, with 17 immigrants.

Respectfully,

R. H. VON EZDORF,
Assistant Surgeon, U. S. M. H. S.

The SURGEON-GENERAL,
U. S. Marine-Hospital Service.

ENGLAND.

Report from London—Plague at Cape Town and other places.

LONDON, ENGLAND, *July 20, 1901.*

SIR: I have the honor to report that for the week ended Saturday, July 13, the death rate of the city of London was 14.2 per 1,000. There were no deaths from any quarantinable disease, but on the preceding Saturday there were 12 cases of smallpox and 1 of typhus fever under treatment in the hospitals. For the same period there was 1 death from smallpox in Glasgow and 1 also in Liverpool.

The report of plague from Cape Colony continues encouraging. Up to July 6 there had been a total of 727 cases with 330 deaths in Cape Town; in Port Elizabeth 20 cases and 9 deaths, and in all other ports of the colony 10 cases and 2 deaths.

In Mauritius, for the week ended July 11, there were 2 fatal cases of plague.

For the week ended July 7, there were in Egypt 14 cases of plague and 7 deaths. Of these, 9 cases and 6 deaths occurred at Zagazig, 3 cases and 1 death at Alexandria, and 2 cases at Port Said.

In Hongkong, for the week ended July 13, there were 26 cases and 21 deaths from plague, showing a great reduction in the number of cases.

Respectfully,

A. R. THOMAS,
Passed Assistant Surgeon, U. S. M. H. S.

The SURGEON-GENERAL,
U. S. Marine-Hospital Service.

Report from Liverpool.

LIVERPOOL, ENGLAND, *July 23, 1901.*

SIR: I have the honor to make the usual report for the week ended July 20, 1901. During the week I inspected 750 passengers for Canadian ports. I advised the rejection of 8 for favus, 2 for trachoma, and 1 for Pott's disease.

Respectfully,

JOHN F. ANDERSON,
Assistant Surgeon, U. S. M. H. S.

The SURGEON-GENERAL,
U. S. Marine-Hospital Service.

GERMANY.

On the diagnostic and therapeutic value of tuberculin.

[By Prof. ROBERT KOCH—Read at the British Tuberculosis Congress, London, July 23, 1901.]

When in the year 1890 I made my first exact communications regarding tuberculin, I was able to point to two important properties of this

medicament, namely, its power to produce specific reactions in persons suffering from tubercular disease and its therapeutic efficacy if used for a length of time.

With reference to the former property, I expressed myself in the following words: "I believe I do not go too far when I assume that the medicament will in future be an indispensable aid to diagnosis. It will enable one to diagnose doubtful cases of incipient phthisis even when one fails to obtain certain knowledge of the nature of the disease by finding bacilli or elastic fibers in the sputum or by the physical examination."

As to the therapeutic effect of tuberculin I said on that occasion: "The main thing in the new treatment is that it begin as early as possible. The incipient stage of phthisis is to be its proper aim, because it is against that it can fully develop all its power." And in another place, "After this experience I am disposed to believe that incipient phthisis can be cured by the medicament with certainty." Since that time, I have had very frequent opportunities of testing the efficacy of tuberculin, and have invariably been able to convince myself anew of the correctness of the statements I made then. I therefore still adhere to the opinion that tuberculin is an indispensable aid to diagnosis and a very effective remedy for incipient phthisis. In proof of the diagnostic value of tuberculin I point to its extensive use as a means of ascertaining tuberculosis in cattle. According to the calculations which Voges had collated from numerous reports, and according to the careful investigations of Eber, it gives, if properly applied, correct diagnoses in 97 to 98 cases in 100. Considering that the diagnosis is made by a single injection of tuberculin, and that errors caused by accidental rise of temperature due to other causes are not entirely excluded, this is a splendid result. Moreover, the injection of tuberculin into cattle, of which we may safely say that it has now been performed in millions of cases, has shown that it is absolutely free of danger; at least, not a single case of its having caused any injury to health has ever come to my knowledge. In the case of human beings, however, the conditions of the diagnostic use of tuberculin are considerably more favorable, for it is not necessary to extort the diagnosis by a single injection, and, therefore, we need not give so large a dose, or produce a strong reaction. On the contrary, we may rest content with a quite slight reaction, but must then repeat the test, in order to exclude the possibility of error. For this purpose I use the following method: In the first place the patient's temperature is observed for at least one day, or better, two, in order to ascertain whether the temperature is below 37° C. Patients whose temperature is above 37° C. are unsuited for the diagnostic application of tuberculin, and ought not under any circumstances to be subjected to the tuberculin test. If a patient is found suitable, he receives an injection of tuberculin under the skin of the back in the forenoon. With weak patients one begins with one-tenth of a milligram; with robust ones whose tubercular alterations are probably very slight, one may begin with 1 milligram. If there is no rise of temperature at all, one gives a dose double as large as the first, not on the next day, but on the day after the next. But if there is a slight rise of temperature, only a quarter of a degree for instance, the dose is not raised, but repeated as soon as the temperature has gone down to its normal level. It very often happens that, though the same dose has been given, the second reaction is stronger than the first. This is quite specially characteristic of the effect of tuberculin, and may be

regarded as a quite infallible sign of the presence of tuberculosis. But if the first small doses produce no reaction, one gives 5 and finally even 10 milligrams. For certainty's sake, I am accustomed to give this latter dose twice, and only when no reaction occurs, even then do I feel justified in assuming that the case is not one of fresh or progressive tuberculosis demanding specific treatment.

If one adopt this method, one will never expose a patient to danger, nor even cause him any serious discomfort, for, as I have already said, slight rises of temperature, up to 38°C ., which give most patients hardly any disagreeable sensation whatever, suffice.

In course of time, I have had a large number of diagnostic tuberculin injections performed in my sick ward on the principles just stated. By the end of the year 1900 the number amounted to 2,890. If I add to these the other cases personally observed by me elsewhere, I get a total of considerably more than 3,000. This is the material from which I derive my experience of the diagnostic value of tuberculin; and I have found that the cases in which, owing to indistinct reactions, a sure diagnosis is impossible are quite exceptional. As a rule, one succeeds either in obtaining distinct reactions or in ascertaining their absolute nonoccurrence. In the former case one may, as is generally known, conclude with certainty that there is a tubercular focus, and unmistakable locally limited symptoms very often indicate its position. The tuberculin test is of special value in judging of catarrh of the apex pulmonis without tubercle bacilli in the sputum, especially when influenza is prevalent, because then cases pretty often occur which, to the eye of the clinical observer, exactly resemble incipient tuberculosis, but are not that. Among our numerous patients suffering from catarrh of the apex pulmonis there were almost 15 per cent in whom no reaction took place; and in all the cases that could be observed long enough the further course of the disease confirmed our opinion that they were not cases of tuberculosis.

Every case of pleurisy, too, as soon as the fever is completely gone and when there is no sputum containing tubercle bacilli, ought to be tested with tuberculin. Of our pleurisy patients, 73.2 per cent reacted. I wish expressly to add that I have never seen any disadvantageous effect of tuberculin when it was used in the way I have described. The conviction that pulmonary tuberculosis in its earliest stages is curable, and that it is, therefore, specially necessary to recognize tuberculosis with certainty in its first beginnings, has been continually gaining ground of late, and the use of tuberculin as a means of diagnosis has again become more and more frequent. In several German sanatoria, for instance, its application has been introduced, and I believe that just these establishments, to which it must be a matter of very great importance to get their tubercular patients in as curable a condition as possible, will find the tuberculin test to be an aid of quite special value.

I come now to the therapeutic value of tuberculin and of this too I maintain that it is completely proved, provided—and on this I have insisted from the first—that its application be restricted to still curable cases—i. e., to those which are not yet too far advanced and not complicated with other morbid processes caused by streptococci, staphylococci, pneumococci, influenza bacteria, etc. As these processes are almost always accompanied by rises of temperature, the best way of guarding against the misapplication of tuberculin is to use it only in cases in which the temperature of the body does not exceed 37°C . That tuberculin exercises an exceedingly favorable influence on all such cases, and even completely cures them as a rule, is a fact of which I

have repeatedly convinced myself, and a number of other medical men who have studied the therapeutic value of tuberculin for years, and have either published their experience of it themselves or communicated it to me privately, have arrived at the same result. As such I name Spengler, Turban, Petruschky, Krause, Thorner, Heron, Rembold, Bandelier, Goetsch, Kirchner, and Kartulis, to whose publications I beg to refer you.

With regard to the therapeutic application of tuberculin, however, it is a fact of special importance that the producing of strong reactions, such as were deemed necessary at first, is now generally abandoned. On the contrary, physicians endeavor to keep the reactions as slight as possible, and not to repeat an injection of tuberculin until the preceding reaction has completely passed off, and the temperature has been normal again for one or even several days. One can even, as Goetsch has shown, carry out the treatment without any reaction at all. It is also very expedient to repeat the treatment with tuberculin, with intervals of three to four months, till the capability of reaction is permanently extinct. Petruschky, who has given this method the name of "stage treatment," has effected by it cures whose permanence has stood the test of years of observation.

The rules which experience has prescribed for the treatment with tuberculin may, therefore, be briefly summarized as follows:

(1) Only patients that have no fever, and in whom the process has not advanced too far, are suited for the treatment. (2) One begins with a very small dose, and increases it so slowly that only very slight reactions, or even none, take place. (3) If reactions take place, tuberculin must not be injected again till the temperature has been normal for one or several days. (4) The treatment with tuberculin must be repeated until, after an interval of three or four months, the capability of reaction is permanently extinct.

Malaria.

[By Prof. ROBERT KOCH—Read at the British tuberculosis congress in London July 23, 1901.]

When I was a student of medicine, we were taught that malaria was not an infectious disease—i. e., not transmissible from person to person, and that it was regarded as the type of the miasmatic as contrasted with the contagious diseases.

What a change has taken place since then! Now we know that malaria can be transmitted when one injects the blood of a malaria patient into the body of a healthy person, a case, indeed, which does not occur under natural circumstances. We know, too, that malaria is not caused by gaseous substances, but by micro organisms, which belong to the category of the animal parasites, are imbibed by gnats with the blood they suck, further developed in the bodies of the gnats, and, finally, inoculated into healthy human beings again. So, according to the views now prevalent, malaria can not possibly be produced without the cooperation of two factors, namely, the malaria parasites and the gnats. It is a matter of course that so complete a transformation of our views as to the nature of malaria has led to a corresponding transformation of opinion regarding the measures to be taken against it. In former times they knew only one means of getting rid of malaria, namely, the drying up of swamps, a means which unfortunately admits of application only to a comparatively small extent. Attempts were,

therefore, made to support this measure by planting certain plants, such as the eucalyptus and the helianthemum, of which it was believed that by their large consumption of water they could deprive the soil of its swampy character. Now a number of other measures, based on better knowledge of the etiology of malaria, have been proposed, of which the following are the most important: First, people are advised to avoid the neighborhood of malaria patients, and to fix their abodes at least 5 miles from places where malaria prevails; second, it is proposed to exterminate the malaria-transmitting gnats by destroying their larvæ at the easily accessible breeding places; third, human beings are to be protected against the gnats by wire nets for their dwellings and by gloves and veils for their hands and faces; fourth, efforts should be made to exterminate the malaria parasites by the rational use of quinine. It is obvious that these four proposals are theoretically of equal value. If one never has occasion to go near people suffering from malaria, or if one is never stung by gnats, one can not get malaria; and if either the gnats or the malaria parasites are exterminated, malaria must vanish forever, because one of the factors absolutely necessary for its production has ceased to exist. How these proposals will stand the test of practice, however, whether it is possible to carry them out to so general an extent as we had to suppose in estimating their theoretical value, is quite another question. The discussion of this question is of special interest at present, because experiments are now being made everywhere with a view to proving the practicability of the measures I have mentioned. I believe, therefore, that I may count on your assent, if I choose the said measures for the theme of this lecture, and take the liberty to discuss the measure proposed by myself in somewhat fuller detail.

If we begin with the removal of swamps, a measure which has been known from of old, we must regard its efficacy as confirmed by manifold experience, and, consequently, as proved. Now, indeed, we know that the effect is not due, as was formerly supposed, to the prevention of the rotting of vegetable matter which was supposed to emit the gases that caused the disease, but to the destruction of the breeding places for the gnats. So, strictly speaking, this measure coincides with that aimed directly against the larvæ of the gnats, of which I shall speak later on. Formerly, attention was paid almost exclusively to extensive swamps, which could be dried up by regulating rivers and by means of deep ditches through which the water could flow off. In this way, districts have not infrequently been freed of malaria. In those cases, however, the purpose was always gained by very expensive works, of which it can not even always be said that they were necessary, for it has been discovered that the anopheles gnats, which are the chief transmitters of malaria, have their breeding places much more frequently in little puddles and pools than in great swamps. Several times in New Guinea I saw many larvæ of anopheles gnats in quite small gatherings of water in wheel marks or even in water butts. In Italy I repeatedly found them in water vessels which were placed in gardens for the purpose of watering the plants. In future, then, it will be necessary to pay attention to the small and even smallest gatherings of water, which can generally be rendered harmless by covering them with earth or by frequent emptying rather than to large swamps, which can not, as a rule, be easily got rid of. Where such easily remedied conditions exist, it is certainly advantageous to remedy them. But in by far the majority of cases, especially in the tropics, comparatively little can be done against swamps. In tropical regions it will not be possible in the rainy season

even to get rid of all the little puddles that keep continually forming and re-forming. In many districts the gnats find many opportunities of laying their eggs not only in the swamps but also in hollows in the trees, axils, etc., so that they are not exclusively dependent upon the swamps at all. On the tobacco plantation of Stephansort in New Guinea, for instance, there were very many anopheles gnats and also a great deal of malaria, though the whole territory was most carefully drained for the tobacco's sake, and swamps were, therefore, impossible.

In general, then, one may say of this measure that it is in itself a useful one, but that the extent to which it can be applied is but limited. Of the direct extermination of the larvæ of the gnats by destroying the breeding places, pouring petroleum into the water, or other larvicidal means the same may be said. Wherever it is possible it ought to be effected, but that will be only where the gnats have but few, easily accessible, and not too large gatherings of water to lay their eggs in. Such attempts have already been made by Fermi in the island of Asinara, off the north coast of Sardinia, and by Kerschbaumer at Rovigno, in Istria. I suppose you know also that Ross has gone to the west coast of Africa again, to try on a large scale to get rid of and destroy the breeding places of the anopheles. Before the end of this year, perhaps, we shall learn the results of these experiments. I am convinced that they will succeed wherever the conditions I have mentioned exist; but wherever there is tropical vegetation, and especially where rice, which requires permanent irrigation, is grown, I consider it impossible to gain any advantage by this measure.

The proposal to run away from malaria, so to speak, by living at least 5 miles away from all native settlements where malaria prevails, is one with which I have very little sympathy. If the pestilence in question were of short duration, like cholera, plague, typhus, and the like, compliance with such advice might be of use. But flight from so stationary a disease as malaria is equivalent to final renunciation of the most fertile regions in the tropics. For this reason, too, the merchants have already declared against it, because it would intolerably hamper all intercourse with the native population. The missionaries, too, are against it, because, if they lived far away from the natives, they would lose their influence over them. And yet this proposal, too, may be advantageously complied with when contact with a malarial native settlement is temporary, on expeditions for instance. In such cases it will always be very expedient to encamp for the night not in or beside the dwellings of natives, but at a suitable distance from them. In permanent settlements, however, only a few individuals will be able to comply with this proposal. Nor has hitherto, so far as I am aware, any use been made of this measure.

The proposal to protect the inhabitants of malarial districts at night against the gnat's stings by nets, veils, and gloves sounds very plausible at first. It has been received with great enthusiasm, and has been acted upon in Italy at many places, and, to all appearances, with good success. But in the case of this measure, too, reasonable as it looks from the theoretical point of view, it was soon found that it admits of but very limited application in practice. In Italy, at least, the arrangements for securing houses against malaria seem not to have been resorted to yet, except in the case of the railway signalmen's cottages and some small railway stations; and as to the wearing of veils and gloves, it may, perhaps, be practicable in the Italian climate, but in the tropics the number of people that will willingly adopt this measure is not likely to be great. The few experiments that have hitherto been made in the tropics with

metal nets for the protection of houses against gnats have, so far as my information goes, given little satisfaction. But even if they should work perfectly at first, I fear they will soon share the fate of the well known mosquito nets, which, in the hands of the native servants, almost always become so defective that they afford but a partial protection against the gnats, if any. For these reasons, and because it certainly can not be made available for the native population, and therefore can not effect a real stamping out of malaria at all, I expect less of this measure than of all the rest.

I come now to the measure proposed by myself, which aims at exterminating the malaria parasites in man by means of quinine. In making this proposal I presuppose two things—first, that the malaria parasites are restricted to man, and, second, that we can destroy them, or at least render them harmless, by means of quinine. As to the first of these two presuppositions, I regard it as adequately proved by the fact that nobody has yet succeeded in finding parasites identical with the human malaria parasites in the blood of any animal. Just as little has anyone succeeded in artificially transmitting human malaria parasites to animals. The second presupposition is proved by the observation that may be made in medical practice every day, that when quinine is properly used the malaria parasites disappear from the blood of the patient. This fact, it is true, does not afford certainty that they really are destroyed; they may only have disappeared from the circulating blood, but remained in the internal organs, especially in the spleen and the bone marrow. And in fact this is mostly the case after a single treatment with quinine, as the extreme frequency of relapses proves; the malaria parasites are not got rid of till after the treatment with quinine has been continued for a length of time. But as soon as one knows this effect of quinine on malaria parasites one will, of course, not restrict oneself to a single application of quinine, but will continue to give it in suitable doses till the parasites are really killed or have died. For our preventive purposes, however, the banishing of the malaria parasites from the circulating blood suffices, because it is only with the blood that the gnats can suck them in.

The first part of the measure, accordingly, is that an opportunity must be given to every person attacked by malaria to get rid of the parasites by means of quinine. In the case of educated and well-to-do people this will be no difficult task, especially when medical aid is to be had. But the treatment of the poor, and especially the natives in the colonies, will not be so simple a matter. If the treatment of quinine entail trouble, still more if it entail expense, those people will rather endure malaria than comply with our demands as to the use of quinine. So the only way is to make the acquisition of quinine as convenient and cheap for them as possible. For this purpose many quinine dispensaries should be established in all malarial districts and in the colonies, especially in the immediate vicinity of the European settlements, where they could get quinine at a very low price, or, much better still, for nothing.

Such dispensaries already exist in British India, where quinine is to be had at a low price at the post-offices. In Dutch India, quinine is given in great quantities gratis to natives and Europeans alike. In Italy a law was enacted lately prescribing the gratuitous dispensing of quinine to workmen attacked by malaria. In this way care is taken not only that the poor get quinine, but also that they get it in a pure and reliably effective form, whereas hitherto, as has been repeatedly proved, it is just into the hands of poor people that quinine has got, by the illicit trade carried on by druggists, in an adulterated condition.

If, however, we should confine ourselves to the gratuitous dispensing of quinine on as wide a scale as possible, we should gain our end only very slowly and perhaps not completely. Only a part of the malaria parasites would be destroyed in this way, for not only persons evidently suffering from the well-known fever attacks which are characteristic of malaria have the parasites in their blood, but also those who suffer from the chronic forms of malaria, with very indistinct and often hardly perceptible symptoms. I have proved, moreover, by very comprehensive investigations, that in the malarial districts proper hardly any of the inhabitants have malaria parasites except the children and those who have immigrated from unmalarial districts. Hitherto, however, the malaria of children, and especially of the native children, has had hardly any attention paid to it; at any rate, nobody has attempted to free the children of their parasites, though, as regards the dissemination of malaria, they are just as dangerous as adult patients with clinically recognizable symptoms.

If, therefore, we wish to render all, or as nearly as possible all, parasites innocuous by quinine, we must take chronic sufferers from malaria and also the children into account. But the only way to gain this end is to examine the blood of all persons suspected of malaria with the microscope. In all the attempts I have hitherto made to exterminate the malaria parasites, I have acted on this principle, and have been able to convince myself that the execution of this measure is not so difficult as it may at first sight appear. Medical men who have evidently never made any attempts of this kind have reproached my method with being too expensive and troublesome, on the ground that the continual examination of the blood and treatment of patients would require too many doctors. But this is by no means the case.

Experience has shown that my method can be carried out even by a very small number of doctors. The taking of blood for examination is so simple and purely mechanical a matter that no doctor is needed for it. In many cases I had the blood preparations made by sick nurses of both sexes and other nonmedical persons, and the results were very satisfactory. I intend to go even a step further and have the microscopic examination of the preparations done by nonmedical people. I have already done so several times with very good results. A female sick nurse, a missionary, and a hospital orderly have been instructed in the microscopic investigation of blood for malaria parasites, and have learned the work so well in a short time that one can perfectly rely on their diagnosis. So it is not at all necessary for a doctor to do all the work himself; he can employ cheap and sometimes volunteer assistants, whom he has only to supervise and inspect. With a sufficient staff of such assistants a single doctor will be able to rule a pretty large malarial district and rid it of the parasites. Nor need the apparatus requisite for the diagnosis of malaria be at all expensive. The well-known optical workshops of Zeiss and Leitz have recently produced small microscopes, perfectly sufficient for this purpose, costing £15 to £20. The cost of the other utensils, such as cover glasses, object glasses, coloring matter, etc., is also very small.

Apart altogether from the measure proposed by me I regard such thorough investigations of the population in malarial districts as absolutely necessary. There is no other way of getting a sure knowledge of the state of malaria there. I had an opportunity of convincing myself of this once more lately in investigating several places in Istria. They were small places, two of which, Punta Croce and Ossero, lie at the

southern end of the island of Cherso ; two others, Stignano and Fasana, are near the town of Pola ; a quite new settlement is in the Brioni Islands. Now in these different places the malaria corresponds exactly to the traffic that goes on in them. At Punta Croce and Ossero, which lie at a distance from all traffic, only the children have malaria parasites in their blood—i. e., the state of malaria was exactly the same as I found it in the coast villages of New Guinea and in remote places in Java. This proves that, in a temperate climate too, malaria, if left entirely to itself, becomes a children's disease ; people get over it in childhood, become immune after some years, and never suffer from it again.

At Stignano, too, it was mainly among the children that malaria was found, but the parasites were detected in the blood of older people, though in small numbers. This is evidently connected with the circumstance that, owing to the nearness of Pola, the population of this place is not so entirely cut off from all traffic as those of Punta Croce and Ossero. The population of Fasana is still more fluctuating, and consequently malaria is still more frequent among the older people. This is most strikingly the case, however, at Brioni, where the population consists almost entirely of workmen who are employed in making the land arable, tending the vineyards, and building harbor works and houses, and who come to the place and leave it again in swarms. They come from the most different parts of Dalmatia and from the mountainous districts of Istria, which are free of malaria, as well as from the coasts of that country, where it prevails. Those that come from unmalarial places almost all get malaria at Brioni, the consequence of which is that the majority of the malaria patients there are adults. To these examples I may add that of Peroi, a coast village north of Fasana. Among 219 inhabitants there only 3 malarial patients were found, and they had evidently been infected elsewhere. This marks it as a place free of endemic malaria. Among the places just enumerated, then, we find all the main types of the varied behavior of malaria: Peroi, without endemic malaria, has only introduced cases; Brioni, with a strongly fluctuating population, has malaria mainly among the adults; Fasana and Stignano show the transitions to the purely endemic behavior of malaria at Ossero and Punta Croce, where it is exclusively a children's disease.

A very interesting and practically important fact was strikingly observable at Fasana and partly at Stignano too; the malaria cases were specially numerous in certain houses and groups of houses, and these were in the periphery of the place, whereas, the center was almost free. I had had occasion to make the same observation before at the town of Grosseto in Italy. From this we may conclude that the infecting mosquitoes do not fly anywhere and everywhere or disperse equally over a place, but have certain predilections. Now, in combating malaria, it will be very advantageous to find out what places they prefer and to pay special attention to such. From this focal behavior of malaria, I drew the practical conclusion that it is not necessary at the outset to free whole places or extensive regions of the malaria parasites. It will be perfectly practicable to advance step by step, in exact accordance with the number of assistants at disposal, without having to fear that the ground just freed of malaria will be at once reinundated by infected mosquitoes from the still malarial neighboring districts.

I now come to the question as to the best method of removing the parasites permanently from the blood of malaria patients by treatment

with quinine. With a view to deciding this question I have made very many experiments, and have arrived at the following results, which, for the rest, every observing physician who has frequent opportunities of treating malaria patients will find confirmed by his own experience. Doses of quinine of less than 1 gram are insufficient for adults. The effect of the quinine is very greatly strengthened by giving full doses several days running. Considering these two facts, and in order to minimize the use of quinine, I order 1 gram of quinine to be given two mornings running, which is repeated after an interval of nine days. This treatment must be continued for at least two months, or better, three, because one is not safe till then against relapses. In obstinate cases one gives 1 gram of quinine three days running, and reduces the interval, if necessary, to seven days. In quartan, which is well known to be the most obstinate form of malaria, quinine must be given three days running from the first. There are people with whom quinine does not agree if taken through the mouth; in such cases it must be given under the skin. The patient must be observed for a length of time after the treatment, and his blood must be examined from time to time, in order that one may be quite sure that he is permanently cured and free of malaria parasites.

In severely malarial districts the combating of malaria will restrict itself in the main to the treatment of the children and of the persons who have immigrated in the immediately preceding years. To children under 6 months one generally gives one-tenth of a gram, to older ones, more, according to their age. They generally stand quinine very well, even in comparatively larger doses than adults, so that one need not hesitate to give children of 5 to 6 years half a gram. They do not dislike it either, if given as a powder mixed with raspberry sirup, or if sweet tea or the like is given after it. If necessary, one can give enquinine, which, however, unfortunately can not be used much, owing to its high price. For the rest, the treatment of children suffering from malaria is one of the most grateful tasks for a physician. When I arrived at Stephansort in New Guinea, there were no children there. They had always died of malaria. I took special pains to protect the children that were born during my stay there, and those that came to the place with their parents, against the pernicious influence of malaria. They were all examined from time to time for malaria parasites, and treated with quinine, if any were found. Under such treatment those children, whose number amounted at last to about a dozen, thrived splendidly; not one of them died.

The practicability of my method was proved by an experiment I made at Stephansort in New Guinea. It is a settlement of the New Guinea Company, with 734 inhabitants. Of these, 157—i. e., 21.4 per cent, were suffering from malaria. This figure was soon reduced to a small remainder, consisting exclusively of quartan cases, and this favorable result was not a merely temporary one, but lasted until the date of the last news I received.

Further experiments testing the practical efficiency of my method are going on at this moment in German Southwest Africa, and in the Brioni Islands, and experiments are to be begun soon in East Africa, and the former ones continued in New Guinea.

At bottom, however, no further proofs of the value of my method are at all necessary, for the results of the extensive and successful attempts to stamp out malaria are already at our disposal. You are well aware that malaria was very prevalent in most European countries only thirty to forty years ago. Since then it has very rapidly dimin-

ished, and now it has nearly everywhere wholly or almost wholly disappeared. What I have just said is especially true of England, France, Belgium, Holland, and Germany. Attempts have been made to explain this very striking decrease of malaria by the drying up of the swamps, but this explanation is by no means admissible. There still are swamps enough everywhere, and the transmitters of malaria, the anopheles gnats, are still to be found in large numbers wherever malaria used to be. So there must be some other reason, and the only other reason discoverable is the much more general use of quinine, which is the only deadly weapon we have against the malaria parasites. Quinine used to be so dear that only well-to-do people could get it. Moreover, since its use became more frequent, the doctors have learned to use it more rationally. So the number of malaria cases that were properly treated and permanently cured became greater and greater, whereas, formerly every case was followed by endless relapses. Consequently the infectious matter has become very rare, and the anopheles gnats, which are probably just as numerous as they used to be, no longer find any malaria parasites to transmit.

The prevalence of malaria in Germany only thirty years ago, and the extent to which it has diminished since then, is best shown by the statistics of the German army. In 1869, the number of cases still amounted to 54.5 per 1,000; now it is .45 per 1,000—that is, it is more than 100 times rarer now than then. In 1874, the garrison of Spandau, a fortress near Berlin, surrounded by swampy meadows, had 664 cases of malaria per 1,000 men; now, though the swamps are just as they were, the figure is one-half to 1 per 1,000.

In Batavia and other towns in Dutch India, which used to be notorious for their malaria death rate and were called “the European’s grave,” a considerable improvement has taken place since the gratuitous dispensing of quinine was introduced.

A very interesting illustration of what I am now saying came to my knowledge lately at Pola, the principal seaport of Istria. Being also a war port, it has a larger garrison, and it has from of old had the reputation of being severely infected with malaria. In 1864 the marines there had 887 cases of malaria per 1,000 men. But the state of things gradually improved, and malaria has considerably diminished since then. In the last few years the number of malaria cases in the same part of the garrison has been only about 30 per 1,000—that is, only one thirtieth of what it used to be. In this case, also, people were disposed to ascribe the improvement to the drying up of two swampy meadows near the town, which was effected in the years 1868 to 1870. But apart from the fact that the decrease of the malaria was not simultaneous with the draining of the swampy meadows, but took place quite gradually and equably in the course of the last thirty years, at about the same rate as in the German army, there is another circumstance which speaks very decidedly against the casual connection between the decrease of the malaria and the draining of the meadows, namely, the following: While malaria has been diminishing in the garrison it has been increasing among the civil population, which is no more and no less exposed to the influences of the climate and the soil, including the swamps near the town, than the garrison. In 1890 the number of malaria cases in Pola and its suburbs was 24.8 per 1,000; since then it has risen to 132.5 in the year 1900—that is, more than five-fold, and that not suddenly but quite gradually. In the same period the number of cases in the garrison has gone down to one-third. So here we have the striking phenomenon of an increase of the number of

cases in one part of the population and a decrease in another in one and the same place. This can be due only to some difference in the circumstances of these two parts of the population, and the difference is that the garrison is under continual medical supervision, so that every case of malaria is at once properly treated, whereas among the civil population, for which medical assistance and quinine are too expensive, this is not the case.

Of reasons, then, that speak for the practicability of the measure proposed by me in different climates, under different social circumstances, and on whatever scale one likes, there is certainly no lack. Nevertheless, I do not ask you to come to a decision just at this moment when, as already stated, experiments testing the value of the various methods proposed are everywhere being made. In a few years the practical results of these experiments will be known to us, and then you may act on the good old saying: "Prove all things; hold fast that which is good."

GUATEMALA.

Report from Livingston—Fruit port.

LIVINGSTON, GUATEMALA, *July 22, 1901.*

SIR: I have to make the following report of the conditions and transactions at this port during the week ended July 22, 1901:

Present officially estimated population, 3,000.

Number of cases and deaths from yellow fever during the week, none; number of cases and deaths from smallpox during the week, none; number of cases and deaths from typhus fever during the week, none; number of cases and deaths from cholera during the week, none; number of cases and deaths from plague during the week, none; number of deaths from other causes during the week, 1. Prevailing diseases, bilious-intermittent fever of mild form. General sanitary condition of this port and the surrounding country during the week, good. Temperature, 80° to 90° F. Rainfall is abundant.

Bills of health were issued to the following vessels: July 8, steamship *Managua*; crew, 16; number of passengers from this port, none; number of passengers in transit, none; pieces of baggage disinfected, none. July 18, steamship *Bergenser*; crew, 17; number of passengers from this port, none; number of passengers in transit, none; pieces of baggage disinfected, none.

Respectfully,

W. K. FORT,

Acting Assistant Surgeon, U. S. M. H. S.

The SURGEON-GENERAL,

U. S. Marine-Hospital Service.

HONDURAS.

Report from La Ceiba—Fruit port.

LA CEIBA, HONDURAS, *July 21, 1901.*

SIR: I have to make the following report of the conditions and transactions at this port during the week ended July 20, 1901:

Present officially estimated population, about 3,000.

Number of cases and deaths from yellow fever during the week, none; number of cases and deaths from smallpox during the week, none; number of cases and deaths from typhus fever during the week, none; number of cases and deaths from cholera during the week, none; number of cases and deaths from plague during the week, none. Prevailing dis-